follow out the idea in 1896, as did Adam Sedgwick in 1899. If authority be necessary, here is authority in plenty. I also tried in my very humble way, beginning as long ago as 1906. I daresay each man in turn thought he was propounding something new. Dr. Cunningham perceives, I hope, that Prof. Goodrich is spared the disgrace of being my pupil, and that even the most self-respecting biologist may, in this instance, follow the truth without qualms of conscience. Dr. Bather knows with what reception I met. I was told that I was doing harm, that biologists could manage their affairs quite well without my help, and so forth. Then the worm turned. So far as I am able to judge, Dr. Bather objects to my letters because they are tediously long and because they are impudent. Certainly they are long, and doubtless they are tedious. But I could state, or assume, in half a dozen words a fallacy which Dr. Bather could not refute in less than half a dozen columns. Moreover, as Dr. Bather courteously indicates, it has been holiday time, during which one does unusual things; therefore I have used his letters —with all reverence, as a parson might—as texts whereon to hang admonitory discourses. Certainly these letters have been impudent—most impudent. But here, again, we have the trodden worm.

Dr. Bather thinks I ought not to discuss variations unless I first account for them, which is like saying I ought not to eat my dinner unless I first cook it. Must I not accept the given fact? I am at once accused of being tediously long and not long enough. What is a poor man to do? Besides, I have tried elsewhere ("The Laws of Heredity," chap. 5) to do this very thing. Primarily variations can arise only in two ways. Either they are impressed on the germ-plasm by its environment, or they occur because the germ-plasm is a living, growing, changing thing which, like other living things, tends to revert to the normal from impressed change, especially injury. There exists ample crucial evidence to enable us to reach a decision, but much of it lies, outside the high roads followed by biologists, in the realms of disease and bacteriology.

Dr. Cunningham's letter (NATURE, November 17, p. 368) is addressed especially to Prof. Goodrich, who may deal with it if he desires; but one passage refers to my particular hobby. Lamarck called the changes which result from use "acquired"; but, thinking only of trifling changes which occur at the end of the development, he did not realise that the growth of the higher animals, especially man, is due mainly to that functional activity which begins to act immediately after birth. His successors employed the word "acquired" as indicating any character which develops under any very glaring influence. Now Dr. Cunningham defines an acquired character as a "change" (from the person's antecedent self, from the parent, from the race—which?) due to "environment or modification." In that case the English language is not "acquired," but is "innate" in an Englishman. If learned by a Frenchman, it is acquired. Heaven knows what it is if learned in Jersey. He accuses Prof. Goodrich and me of a "misuse of words" and of obscuring "a *perfectly clear* distinction"! The italics are mine!

G. ARCHDALL REID.

9 Victoria Road, Southsea, November 19.

The Softening of Secondary X-rays.

DR. A. H. COMPTON in a letter on this subject in NATURE of November 17, p. 366, described an experiment in which he reflected the Kα rays from a molybdenum Coolidge tube on to a slab of paraffin, and measured the absorption coefficient of the secondary scattered rays at different angles with respect to the direction of the primary beam. The absorption coefficient of the secondary rays was found to be 29 per cent. greater than that of the primary Kα beam at $\theta=90°$, and 6 per cent. greater at $\theta=20°$. This softening of the rays on being scattered was still more pronounced when the K lines of tungsten were used.

Dr. Compton referred to this work as a repetition of measurements which I had previously reported (*Phil. Mag.*, September, 1921), in which no such increase in absorption after scattering was observed, and he attributed my negative result to an unfavourable choice of wave-length and angle. Apparently he did not understand the purpose of my experiment. It was to settle a question regarding the interpretation of energy measurements made with the Bragg spectrometer. We were not sure that the atom in a scattering substance does not always absorb energy from the incident rays and re-emit this energy in a manner characteristic of the atom and independent of θ. My problem was to find out if such an effect need be considered in ordinary spectrometer measurements. The wave theory of scattering predicts a certain amount of softening due to the finite size of the atom and to a sort of Doppler effect, but not nearly the observed amount, especially at large angles.

As Dr. Compton suggests, there is probably an additional somewhat softer radiation due to collisions of electrons released within the scattering substance by the primary rays. Such a "fluorescent" radiation should diminish with θ, as observed. The softening due to the finite size of the atom should also, in general, diminish with θ and be negligible in the characteristic radiation, which is believed to consist of relatively sustained wave-trains. Softening due to these recognised causes can thus be minimised by using the sustained characteristic rays, large wave-lengths, and θ as small as possible. I chose these conditions, which were unfavourable to the Compton effect, because I wanted to eliminate it so far as possible. The negative result simply indicates that with light atoms the indirect unpolarised radiation sought is not great enough to require consideration in ordinary crystal measurements.

S. J. PLIMPTON.

Worcester Polytechnic Institute, Worcester,
Massachusetts, November 8.

The Molecular Scattering of Light in Liquids and Solids.

As was pointed out by the late Lord Rayleigh, the basis of his theory of the blue sky, namely, that the molecules scatter the incident energy independently of each other's presence, is only true for gases in consequence of the freedom of movement the molecules possess in this state of matter. In connection with the problem of the colour of the sea and of deep waters generally it is necessary to know the scattering power of ordinary liquids, such as water, and I find this can be very simply accomplished by application of the theory of local fluctuations of density arising from molecular movement, originated by Einstein and Smoluchowski and utilised by the latter to elucidate the phenomena occurring near the critical state. The general formula for the scattering power of a fluid is

$$\frac{\pi^2}{18}\frac{RT\beta}{N\lambda^4}\left(\mu^2-1\right)^2\left(\mu^2+2\right)^2,$$

where β is the compressibility of the substance, μ its refractive index, R, T, N being the usual constants of the kinetic theory. The scattering power of water comes out from this formula as about 160 times that

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of air. Not only is this in agreement with observation, but I find the coefficient of extinction of light due to scattering

$$\frac{8\pi^3}{27}\frac{\mathrm{RT}\beta}{\mathrm{N}\lambda^4}\left(\mu^2-1\right)^2\left(\mu^2+2\right)^2$$

closely represents the observed transparency of pure water in the region of the spectrum where there is no selective absorption. Work is now in progress testing the formula in the case of other liquids.

It is clear that an application of the same idea of local fluctuations of optical density and of Debye's theory of the thermal movements in solids would give the theoretical scattering power of transparent crystals for ordinary light. This is also being tested.

C. V. RAMAN.

210 Bowbazaar Street, Calcutta, October 15.

The Tendency of Elongated Bodies to Set in the North and South Direction.

THE letter from Sir Arthur Schuster in NATURE of October 20 last requires amplification and amendment in one particular. The setting tendency of an elongated body depends upon its method of support. If suspended, with the centre of gravity not free to rise and fall, it is at its "lowest" position when lying on the equipotential section of maximum radius of curvature, *i.e.* tends to set east and west. A floating body, on the other hand, where the centre of gravity is free to rise and fall, is at its lowest when lying on the equipotential of minimum radius of curvature, *i.e.* north and south. The whole matter is fully discussed in an article by Mr. W. D. Lambert, of the United States Coast and Geodetic Survey in the *American Journal of Science* for September last.

The tendency of the rod of a torsion balance to set east and west was pointed out by Baron Eötvös in one of his early papers, probably one of those presented to the International Geodetic Conference, but I am not able to lay my hands on the exact reference.

E. H. GROVE-HILLS.

Ophion luteus.

IT is a quite common experience to see *Ophion luteus* fly into houses at night attracted by light. I have myself captured at least half a dozen specimens that had in a single hour flown into a room in that way. Not long ago Dr. James Waterston, at my request, dissected a fresh female specimen, and found in it a poison gland, reservoir, and duct similar in character to those recorded as being present in certain other species of Ichneumonidæ.

M. R. du Buysson, in a paper (*Rev. d'Entomologie*, vol. 11, p. 257, 1892) which I have only recently seen, states that he had often been stung by Ichneumonidæ of the Ichneumon, Pimpla, and Ophion groups; but however much poison may have been injected, the pain and inflammation produced by the sting, he says, lasted only a short time. He dissected a large number of specimens belonging to all the groups of Ichneumonidæ, and always found one or several poison glands present.

The larvæ of *Ophion luteus* appear to be parasitic in the caterpillars of many different species of Lepidoptera; but it would appear to be the case also that the female does not pierce the skin of the caterpillar to lay her eggs inside. She is said to lay them on the skin. That point probably needs confirmation. But however that may be, it seems to me incredible that the female would mistake the arm of a young lady for a caterpillar. In M. du Buysson's case, the insect had always been held in the hand or otherwise irritated before it attempted to sting, and this seems to be the general experience. He was never puzzled to divine the purpose of the sting, regarding it, no doubt, simply as an act of self-defence; and that is the explanation which I would venture to suggest in reply to Sir Herbert Maxwell's letter in NATURE of November 10.

C. J. GAHAN.

Natural History Museum, S.W.7,
November 14.

IN respect of Sir Herbert Maxwell's letter in NATURE of November 10 on *Ophion luteus*, Linn., may we hope that the insect caught *flagrante delicto* was preserved in order that the species might be placed beyond a doubt, these large red Ophionidæ being almost impossible to differentiate at a glance?

Ophion luteus is apparently a nocturnal insect. I have observed it at night hunting for Dianthœcia larvæ. The species is credited with a long list of hosts, chiefly Noctuid moth caterpillars.

Almost all Ichneumonidæ will "sting" or attempt to if handled, the males included (though, of course, morphologically incapable), but that any member should make an attack unprovoked is most unusual. *O. luteus* and allied species are extremely bad-tempered—a fact which hampers work with them in confinement, as they repeatedly "sting" potential hosts to death without attempting to parasitise them. Perhaps this irritability was the cause of the unusual attack.

R. STENTON.

Pathological Laboratory, Ministry of Agriculture, Harpenden, November 19.

Sex-manifestation and Motion in Molluscs.

I DO not wish to prolong the discussion upon sex-differentiation and mode of life, though I venture to think that Dr. Orton's reply in NATURE of November 3 to my letter in the issue of October 13 leaves several questions very much as they were before.

On many points I find myself in agreement with Dr. Orton, though I consider that the incidence of sex-differentiation in the Mollusca does not exhibit that general correlation with an active habit demanded by Dr. Orton's hypothesis. I quite agree, as I said in my previous letter, that many forms originally considered diœcious *may* be monœcious; but I think it is for Dr. Orton to prove this, and I shall await his demonstration with interest. I would like to point out, however, that it will not be enough to show *sex-change* (a turnover from maleness to femaleness, or *vice versa*). The implications of Dr. Orton's hypothesis entitle me to demand from him something in the nature of *permanent* hermaphrodite forms.

G. C. ROBSON.

Natural History Museum, S.W.7,
November 14.

Sinistral Limnaea peregra.

LAST year I started a breeding experiment with two pairs of sinistral *Limnaea peregra* given me by Mr. J. W. Taylor, of Leeds. The first two generations have not come out on any plain plan, and it is necessary to carry them further if the mode of inheritance of this very rare form of one of our commonest freshwater snails is to be worked out. But the young have now quite outgrown the possibilities of my establishment, and if anyone would take over some of them and breed them out (which is quite simple, as they want little attention) they would be doing me a service.

A. E. BOYCOTT.

University College Hospital Medical School,
University Street, Gower Street, W.C.1,
November 7.

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